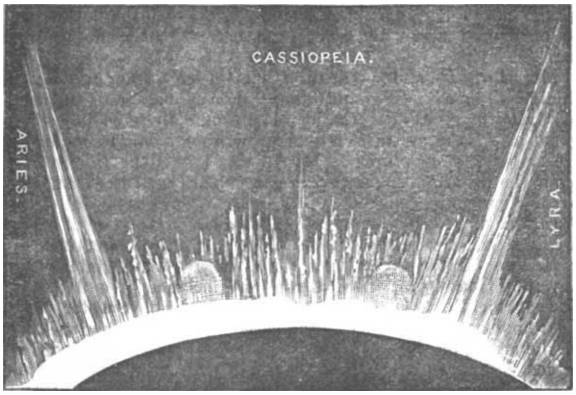
region of facts to that of laws," are no less applicable to physiology than to physics; although Whewell himself (in his "History of the Inductive Sciences") had maintained the contrary. The full acceptance of the doctrine of evolution as our highest expression of the *order* of creation seems to me to lead to a much nobler conception of the Intelligent Cause of that order than any accumulation of such individual adaptations as might be made by the "mechanic-god" of Paley.

WILLIAM B. CARPENTER

56, Regent's Park Road, N.W., November 14

The Aurora and its Spectrum

YOU have already illustrated in NATURE the aurora and electric storm of January 31. Though somewhat tardily, another drawing with a description of this fine display has found its way into my hands, of sufficient interest, I think, to be added to those already published. It comes from Mr. C. L. Prince, the well-known meteorologist of Crowborough Beacon, Tunbridge Wells, who says, "I inclose a photo of my sketch on a slate.[1] I had a splendid sight of the aurora at about 9h. 15m. It soon became very hazy, but I saw it again at 11h. 15m., when I made the sketch (see drawing). The arch was exceedingly well defined, and extended about 80° along the horizon. At 11h. 20m. some more brilliant streamers shot up along the whole convexity of the arch, and the two *patches* of *light* became very tremulous, almost shifting a little from right to left; but I particularly noticed that they did not vibrate *simultaneously*, *i.e.* if one indicated motion the other was quiescent *until* the first had ceased to show excitement, and this action was alternating for nearly an hour. At 11h. 40m. the arch had much contracted, and by midnight had nearly faded away. The whole phenomenon was free from colour. I noticed a few small meteors. The night was quite calm; wind south-west."



This account seems to me interesting in connection with an observation made by my friend Dr. Vogel of Potsdam, that during the aurora of April 9, 1871, certain lines in the spectrum alternated in intensity with the character of the discharge, some brightening as others faded.

Mr. Prince does not mention any spectroscopic observations, and while noting this will you permit me to make a few remarks on the "spectrum of the aurora," an old hobby of mine. I notice that Dr. Spottiswoode, P.R.S., in his lecture delivered before the British Association at York on September 5 last (NATURE, vol. xxiv. pp. 572, 3, "On the Electric Discharge, its Forms and Functions,") has referred to the aurora in connection with experiments showing that the discharge in rarefied gases differs from that at higher pressures, and that the difference corresponds to that observed between the flickering play of the aurora and the crashing spark of the lightning-flash. After then referring to the questions of height and colour of the auroral discharge, Prof. Stokes' theoretic view of the connection of earth-currents, disturbances of the magnetic needle, and solar radiation is dwelt upon. In NATURE, vol. xxiv. pp. 613-18 (lecture by Prof. Stokes, Sec.R.S., in the South Kensington Theatre, on "Solar Physics"), this theoretic view is set forth, and the aurora is described as a flash of lightning passing through the higher regions of the atmosphere where the air is rarefied. There are, I think, some objections to this theory founded upon certain circumstances of the aurora itself, such, for instance, as the well-authenticated cases of auroræ seen close upon the surface of the earth.

The passage, however, in Prof. Stokes' lecture which particularly struck me is this: "But what of the auroræ? It has long been recognised that the aurora is an electrical phenomenon. It has been supposed to be imitated, *and there can be no doubt that the supposition is a correct one* [the italics are mine] by sending an ordinary electric discharge through a highly exhausted tube."

Now it may be true that the aurora is thus imitated so far as external appearance is concerned, and it has long been a favourite idea that this imitation in some way extended to the aurora's actual composition; but what does that Ithuriel spear, the spectroscope, say upon that point—a point which gains the more importance from the fact that such an instrument is mentioned by Prof. Stokes (p. 614) as the true touchstone for the aurora? It says positively that Prof. Piazzi Smyth's citron line, the one true test of the aurora, has never yet been seen in any electric discharge whatever which we have yet produced, whether in air at ordinary pressure, or rarefied; that the red line (its companion in some auroræ) is equally noticeable for its absence therefrom; and that of the remaining faint and less marked lines one or two only have with doubt and uncertainty been by some referred to the air spectrum as excited by the electric spark or glow. The late Prof. Ångström endeavoured to place some of these fainter lines in accord with the spectrum bands of the violet pole in Geissler air-tubes, but the comparison failed on critical examination. Prof. Vogel has also considered the aurora might probably be an air-spectrum modified by conditions of temperature and pressure. The Professor's actual line comparisons, however, quite failed as to the citron and red lines, and could hardly be called a success as to the fainter ones. In fact any analogy between the aurora spectrum and the spectrum of the electric discharge in air is all but hypothetical, and the aurora still maintains that mysterious quality which distinguishes it from electric discharges of all sorts, and indeed everything else, viz. its peculiar spectrum.

I therefore again plead the necessity for spectrum observations in connection with the aurora, a point from which Prof. Stokes' lecture, I am afraid, owing to its absence of any remarks on the subject (save that before referred to) is somewhat calculated to draw them away. It is certainly *possible* that some special gas *may* exist in the upper regions of the atmosphere giving rise to the citron, perhaps the red, lines; but then (as Prof. Smyth remarks) if so, why, being an emission spectrum in the aurora, does it not, according to the theory of exchanges, appear as an absorption spectrum or dark lines in the solar spectrum? Even, too, could this be shown, it would still remain an unexplained fact that such a gas has hitherto failed to be recognised in any other body, celestial or terrestial.

To sum the matter up, the electric discharges in vacuum-tubes, as tested by Prof. Stokes' prism and slit, no more represent the aurora than did the cirrus cloud illuminated by the light of the moon, mentioned by him, which also simulated it.

Upon reliable authority the spectrum of lightning may be considered that ordinarily given by a spark in air; but when we come to rarefied discharges and the aurora, the same comparison does not hold good; and where the electrician has to leave the matter, the spectroscopist has yet to take it up.

To aid in solving the aurora's mystery I would invite all spectroscopists, armed with suitable instruments, persistently to aim at accurate micrometer readings of the aurora spectrum. The approximate places of the lines are pretty well established, but their actual wave-length positions are much wanted, for so only may we hope to master one of the remaining riddles of science.

J. RAND CAPRON

Guildown, November 1

Arctic Research

"PROGRESS of Arctic Research since the Foundation of the British Association," by C. R. Markham, C.B., F.R.S. Such is the title of a very able and instructive paper read before the Geographical Section of the British Association at York on September 6 last, and published *in extenso* in the November number of the *Proceedings* of the Royal Geographical Society. The casual reader of this history may suppose it to be a fair and correct record of half a century of Arctic exploration, and that

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[1] I commend this method of sketching to other observers, the slate pencil showing white on the dark slate being readily photographed with good effect. displaying the aurora light on a dark ground.—J. R. C.

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the names of distinguished men commanding naval expeditions, who themselves, or the officers under them, did a large amount of discovery and good scientific work, are mentioned, however briefly. Yet this is far from being the case. True, some of those expeditions which have been considered unworthy of notice were sent to the far north to gain tidings of the lost expedition under the good and noble but unfortunate Franklin; yet, in addition to doing an immense extent of sledging, by which many hundred miles of *new* coast were traced, they collected much scientific information, little, if any, less valuable than that brought home by the Nares Expedition, whose object was purely scientific.

The names of Kellett, Belcher, and Austin are conspicuous by their absence, except a notice of the officer who acted as second to the latter, and who made a comparatively short sledge journey. We can scarcely suppose that the author was in any way influenced by the fact that he himself sailed in the same vessel under the immediate command of the officer whom he selects for notice, whilst wholly ignoring Admiral Austin, the chief in command.

The names of the Americans, Kane and Hayes, who with very inferior means traced *more* than 300 *miles*[1] of the unknown shores of Smith's Sound, are left out, although each was awarded the gold medal of the Royal Geographical Society—the highest honour that can be bestowed on an explorer—for their Arctic discoveries, as being the most important of the year in which they were made. These are a few examples to prove what has been said.

The author attempts to show that the system of sledging adopted by the Government Naval Expeditions is all but perfect, and that the first long sledge journeys were performed by them. Both these statements admit of question. The sledging arrangements are more or less defective in almost every particular. The tents are bad shelters, far inferior to snow-huts; the sleeping-bags are objectionable, as they prevent transmission of caloric from one individual to the other. The bag itself, being exposed all day to a temperature of perhaps 20° or 30° below zero, is so cold when the occupant gets inside, that all the moisture from the breath, &c., gets condensed upon it, making this bed after a few days' use like "sheet iron" (as remarked by a naval officer who had some unpleasant experience of the fact) and afterwards, when thawed by the extraction of most of the animal heat from the shivering tenant, becomes a veritable "wet blanket." Besides this blanket bag each man before going to rest (?) struggled into what is called a "jumper" made of thick duffle, cold as the other night-gear, thrusting the arms into the sleeves, thus keeping these unfortunate members separated by two folds of a thick non-conductor from the body that owned them, a procedure wholly opposed to both experience and science. To conserve the animal heat the Indian and white *voyageur* either take off their coats or take their arms out of the sleeves when camping out in winter. The Eskimo strips to the waist in his snow hut. It is only the naval Arctic sledge parties that act differently, with very uncomfortable results. The construction of the sledges is very defective for certain conditions of snow when in a semi-packed state—the sledge runners then sink down six or seven inches, and have to be dragged with great difficulty *through* the snow, not *over* it. The dead weight—exclusive of food and fuel—to be hauled on this "admirable system" usually amounted to 85 or 90 lbs. per man, which weight has been reduced under another system to 35 or 40 lbs. per man without loss of efficiency.

"Long-sledge journeys," which Mr. Markham claims to have been initiated by the Government Naval Expeditions, were made at the rate of twenty miles or more a day by civilians, before the "naval system" was put in use, the first occasion of which was by the most experienced of Arctic explorers, Sir James C. Ross, in 1849, when the results were by no means satisfactory, considering the large number of men employed; a small party of five Hudson's Bay men having accomplished more than double the distance at nearly double the daily rate two years before over very difficult ice.

Mr. Markham "formulates three canons," which, he says, "are equally important, as the true methods for future Polar research," but the latter of these, namely, "to reach an advanced position within the unknown area, it is necessary to follow a coast-line trending northwards, *with a western aspect*, is of most interest to the geographical inquirer."

The four words I wish to comment upon are in italics. Had not Mr. Markham given examples of his meaning by mentioning Foxe's Channel and Regent's Inlet, I should have supposed "*western aspect*" a misprint.

It is a curious but certain fact, established by men who have been there, that the shores of all bays and inlets on the northern coast of America, the shores of all inlets and of the great islands lying north of America, having a *western aspect*, are usually ice-blocked, whilst the shores having an *eastern aspect* are comparatively free from ice and navigable. As cases in point Hudson's Bay, Regent's Inlet, Victoria Strait, the shores of Banks Land and Melvile Island, also Smith Sound,[1] are all far more ice-obstructed on the shores having a western than on those having an eastern aspect.[2]

The greater frequency of westerly and north-westerly winds and gales in the western hemisphere in high latitudes are of themselves sufficient to produce the effect I have mentioned, and to form the so-called "ancient ice" in a month or less.

The route by the west shore of Franz-Josef Land is named as favourable for getting far to the north. This may be true, but from the formation of the land it may be considered as by no means safe. The most experienced navigators of these seas state that Franz-Josef Land may perhaps be reached once in every four or five years, as the condition of the ice in Barentz Sea is very uncertain. Supposing this to be true, might not the vessel that was fortunate enough to reach these interesting islands be detained there rather longer than was either safe or agreeable? Already some not unreasonable anxiety is felt for Mr. Leigh Smith in his fine vessel manned by able icemen, who, it is believed, sailed in the direction indicated. Fortunately he is in every respect well provided to face *one* Arctic winter.

That a large quantity of heavy ice is to be generally met with in the Sea of Barentz is not difficult to account for—Spitzbergen on the west, Franz-Josef Land on the north, and Novia Zemblia on the east, form a sort of *cul-de-sac*, into which the floes appear to be drifted about by winds and currents in a most erratic manner.

Mr. Markham states that the "ancient ice" 80 or 100 feet in thickness seen by Nares and other Arctic navigators is the result of "*slow accretions*," meaning, I presume thereby, that these great ice-masses are the gradual growth of a single floe by the freezings of many winters. My opinion is that this thick-ribbed ice is the result of extreme pressure, which has forced one floe over or under the other, as in the case of the upheaval of the Austrian ship with Weyprecht and Payer in 1873-4, of which mention has already been made in the columns of NATURE.

It is doubtful if the extremest cold of an Arctic winter would have any freezing influence on the lower surface of a floe 60 or 80 feet thick, only 9 or 12 feet of which was above water, whilst the remaining 50 or 68 feet were submerged. The known effect of the action of the sea in wasting away the submerged portions of icebergs, even when these are not large, tends to support the theory I suggest.

J. RAE

4, Addison Gardens, W., November 5

A Photographic Experiment with Swan's Incandescent Light

SOME trials I have recently made with Swan's incandescent electric lamps give results that may possibly interest your readers. I employed throughout an electric stream of the same energy—that generated by thirty Grove cells, and as the whole experiment lasted but an hour, it may be assumed, for all practical purposes, that the strength of the current was uniform throughout.

I made use of four lamps in all, supplied promiscuously from Newcastle, their resistances being respectively: No. 1, 67 ohms; No. 2, 59 ohms; No. 3, 58 ohms; No. 4, 52 ohms. My object was to ascertain their actinic power upon a gelatino-bromide film, individually, collectively, and in groups. To do this I exposed a gelatino-bromide film to the action of one light at a distance of 14 feet, the sensitive film being placed behind a screen more or less transparent. The screen was divided into small squares, each representing different thicknesses of fine tissue paper. Thus: square marked No. 1 represented one thickness; No. 2, two thicknesses; No. 3, three thicknesses, and so on;

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[1] This distance (300 miles) of new discoveries is that credited to the Nares Expedition by Mr. Markham.

[1] The author says that Sir G. Nares ascended Smith Sound having a northern trending, but he does not tell us that both on the outward and homeward voyage he kept to the shore having an *eastern aspect*—a fact.

[2] The east coast of Greenland is an exception to this rule, being ice-encumbered by the force of a well known strong current pressing the floes against it.

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